Supreme Court, U.S.
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IN THE

SUPREME COURT OF THE UNITED STATES

October Term, 1987

EUGENE DONG, JR., M.D.
Petitioner,

v.

THE BOARD OF TRUSTEES OF LELAND STANFORD JUNIOR UNIVERSITY, et al.,
Respondents.

ON PETITION FOR A WRIT OF CERTIORARI
TO THE COURT OF APPEAL
OF THE STATE OF CALIFORNIA
SIXTH APPELLATE DISTRICT

PETITIONER'S REPLY MEMORANDUM
APPENDIX D

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Misrepresentation and Responsibility in Medical Research

Robert L. Engler, James W. Covell, Paul J. Friedman, Philip S. Kitcher, and Richard M. Peters

Abstract: Early in 1985, after being questioned about duplicate data in two of his papers, Robert A. Slutsky, M.D., resigned his appointments as a radiology resident (trainee) and nonsalaried associate clinical professor in the Department of Radiology at the University of California, San Diego. During the following year, faculty committees investigated Slutsky's entire bibliography of 137 articles published in 7 years; 77 (including reviews) were classified as valid, 48 were judged questionable, and 12 were deemed fraudulent. The majority of these papers were published while Slutsky was a research or clinical trainee in cardiology, nuclear medicine, and then radiology. Our analysis of this case leads us to conclude that research fraud, although probably rare, in view of the size of the research establishment, may evade detection, and that there are scientists prepared to run the appreciable risk of submitting inaccurate statements for publication.

Sophisticated dishonesty can escape detection by peer review and replication. The emphasis on competition and the pressure to produce, while intended to advance the discovery of truth, may foster a conflict between personal career goals and the intellectual motivation of scientists seek the truth.

The scientific community needs to address the issues raised by recent reports of fraud. Each institution and granting agency must have procedures for investigating suspected fraud or unethical practices, procedures that protect both the person who reports such practices and the accused person from premature disclosure. As we heighten awareness, we must avoid a "witch hunt." Deterrence of research fraud is clearly needed, but institution of practices that might stifle originality or discourage cooperative research would be counterproductive. (N. Engl. J. Med. 1987; 317:1383-9.)

Early in 1985, a member of a departmental ad hoc committee evaluating the appointment of Robert A. Slutsky, M.D.,

as associate professor of radiology at the University of California, San Diego, raised some questions about apparently duplicated data in two articles by Slutsky. Within one month of being confronted with this problem, Slutsky had resigned his appointments as a radiology resident (trainee) and nonsalaried associate clinical professor in the Department of Radiology. In accordance with procedures established by the School of Medicine, an ad hoc committee was appointed to investigate the initial charges. This committee examined recently submitted or published papers and laboratory records and interviewed laboratory personnel and postdoctoral trainees. In the light of the evidence from laboratory logs and the testimony of technicians and coworkers, the committee found that three recent manuscripts contained descriptions of experiments that had clearly never been performed and requested retraction of the two papers that had been published [...]. The committee also recommended a full investigation. Before this investigation was complete, Slutsky's attorney requested retraction of 15 published papers in eight journals, explaining that "information had recently come to Dr. Slutsky's attention which cast some doubt on the results."

During the following year, a committee of 10 faculty members investigated Slutsky's entire bibliography of 137 articles published in seven years [...]; 77 (including 4 reviews) were classified as valid, 48 were judged questionable, and 12 were deemed fraudulent. The majority of these papers were published while Slutsky was a research or clinical trainee in cardiology, nuclear medicine, and then radiology. Examples of fraud include reporting numerous experiments that were never performed [...], reporting procedures that were incorrect or measurements that were never made [...], and reporting statistical analyses that were never performed [...]. Instances in which statistical calculation errors were apparent in the article were classified as questionable [...]. These facts were reported to the National Institutes of Health and other granting agencies and were released to the press and interested persons. Correspondence with coauthors and interviews with senior department faculty disclosed how this young investigator's fabrications had escaped detection for so long.

What is Fraud? Fraud, in our judgement, involved intentional misrepresentation. The perpetrator makes statements that are intended to induce others to believe things that he or she knows to be false. However, fraud is not the only kind of culpable misrepresentation. It is convenient to distinguish three ways in which inaccurate statements may be produced: (1) through justifiable mistakes - cases in which the scientist had no knowledge or basis for believing that the statements he or she was making were incorrect; (2) through careless errors - cases in which the scientist had no intent to deceive by the information would have provided reason to doubt the accuracy of the statements made was available; and (3) through fraud - cases in which the statements made were known by the scientist to be false and in which the scientist intended to deceive others. Justifiable mistakes do not raise the question of culpability. Careless errors and fraud involve a range of culpable actions, from negligence in the supervision of research or the execution or experiments to a clear intent to deceive.

After due consideration of what requirements and standards applied, the members of the second committee adopted the position that the ethos of scientific research requires that hypotheses be validated before they can be accepted and that claims to observation be open to scrutiny by peers. The legal principle of "innocent until proven guilty," which might be rephrased "assume correct until proved wrong," does not apply to scientific work; the burden of proof remains with those claiming new findings. Thus, the authors of a scientific publication that is reasonably alleged to be fraudulent bear the responsibility for establishing the accuracy of their results.

We assume the honesty of scientists until the first breach this trust. Since the first committee had found clear evidence of fraud in three papers, other could be accepted as valid only if other investigators could specifically vouch for the work. Because the committee had no reason to doubt the honesty of the 93 coauthors contacted, it agreed to accept their statements describing personal knowledge of the data from collection through publication as sufficient evidence of validity. When such statements could be made, the committee did not require reexamination of the original data and classified the publication as valid.

A classification as fraudulent required direct evidence of obvious misrepresentation. Otherwise, papers were classified as questionable. Although a questionable paper may not contain false information, the committee reported that accuracy could not be assured, because the committee had no confidence in Slutsky's scientific integrity and not combination of coauthors could vouch for the entire study from valid experiment to accurate report. If Slutsky processed some or all of the data completely independently, then the paper was likely to be labeled questionable.

The classification of questionable validity or fraud in papers with many coauthors raises the issue of the responsibility of the coauthors. Should the coauthors who were unable at the time of the committee review to vouch for parts of articles have had reason to wonder earlier about the accuracy of the statements made in their names? In multi-authored papers, can one expect, for example, the statistician to know exactly how the data were collected? Can a supervisor know what actually went on in the laboratory during every experiment? An unrealistic standard cannot be applied in judging the culpability of the coauthors. But the standard should be higher than that of many of Slutsky's coauthors and coauthors of others found guilty of fraud.1,2 In the absence of any intent to deceive, many have been careless about verifying the accuracy of publications that carry their names. We believe this to be a culpable degree of carelessness. Acceptance of a gift co-authorship (in which no scientific contribution was made) is also a culpable act of deliberate misrepresentation.

Subtle misrepresentation by omission could be a careless error or an act of fraud. Scientific texts are subject to conventions, understood by authors and by readers,1 that do not require the telling of the whole truth of every aspect of what went on in the laboratory (since that is evidently impossible) but that require the provision of complete information about certain facets of the activity. For example, Slutsky sometimes used the same figures in multiple manuscripts without citing the previous article. These could be considered "methods" figures, but the text often implied (without stating explicitly) that the data were from the current study.

Two kinds of motives may have a role in the performance and reporting of scientific work. Researchers are typically moved partly by intellectual motives - notably curiosity and a desire to acquire and disseminate new information. But personal motives - the desire for fame and fortune in their various degrees or for the advancement of some cause - cannot be ignored. Such personal concerns play a productive part in the advancement of science when they lead to actions in accord with intellectual motives. The congruence of personally and intellectually motivated actions benefits both society and investigator. Negligence and fraud arise when this happy congruence breaks down - when personal motives conflict with and overcome intellectual motives and scientific ethos.

It is tempting to explain instances of culpable misrepresentation by the "rotten apple" theory. The scientist who is at fault has subordinated intellectual ends to personal motives as a result of character flaws. Occasionally, fraud may represent a form of unconscious, perhaps self-destructive, behavior that may have aggressive components directed against supervisors, other authority figures, or colleagues.3 On the other hand, the "rotten system" theory suggests that the organization of scientific institutions is responsible. In our judgement, each of these theories is simplistic: broad social factors may generate situations in which personal motives, and the individual characteristics of scientists determine the relative priority they give to personal ends and the means they use to achieve them.

Nobody knows how frequently these conflicts occur. In the performance of research, it is tempting to forget an "anomalous" result, to fail to scrutinize data in a collaborative project, to leave the collection of data to a fast-working colleague known to be "sloppy," or to accept gift co-authorship. Although the ultimate responsibility for avoiding these temptations belongs to the individual scientist, there is much that research institutions and the society of biomedical research can do to diminish the conflict between personal and intellectual motives.

How Is Fraud Detected?

Like Cyril Burt, Slutsky was a victim of his own statistical naivete. A scientist with more knowledge of statistics would have assigned different statistical properties to data sets that

were alleged to be empirically distinct. For several papers, recalculation of statistics with use of the methods cited failed to yield the claimed result. Yet, even with the gross statistical errors, the fraud would not have been unmasked had it not been for the reading of two of Slutsky's papers in quick succession by an astute reader able to spot the coincidence of tabulated values. Arguably, the papers could have been read independently for years without arousing any suspicion of inaccuracy, carelessness or fraud. These examples raise the question of exactly how common fraud is in biomedical science. The traditional reasons for assuming that fraud in science is rare are that the basic motivation of scientists is to seek truth and that detection will occur through peer review before publication or funding, or through failure by others to confirm the results. Reliance on scientific ethos, although presumably justified most of the time, ignores the possibility that personal and intellectual motives may not be congruent. We shall now show that detection by peer review or replication cannot be uniformly reliable.

Peer Review

Plausible, internally consistent fabrications, like those in the work of Darsee and Slutsky, cannot be detected by the peer-review system.2 However, in several of Slutsky's articles (Appendix, 4, 26, and 71) and in some of Darsee's papers,4 reviewers failed to detect incorrect statistical calculations and implausible technical accomplishments (Appendix, 4, 26, and 71). Why was the quality of some of these reviews deficient? In January 1987, a total of 752 articles was published in 26 of the journals in which Slutsky's articles appeared. Each of those articles, plus the articles that were rejected, required review by two or more qualified persons. Are there enough qualified reviewers? The review process is complicated by the sophistication and multiplicity of techniques employed in individual studies, especially in those such as Slutsky's, in which the research involved three clinical specialty areas - cardiology, nuclear medicine, and radiology.

Are we expecting too much of editors in requiring them to locate reviewers who are skilled in statistics, aware of the details of measurement of coronary blood flow with microspheres, and knowledgeable about technical information, such as the resolution of a new cardiac-imaging technique. Although even this

cursory analysis indicates that the peer-review system may be overloaded, we think it is largely effective in detecting internal inconsistencies. Such inconsistencies were rare in Slutsky's work. However, the failure to detect them in several cases indicates that the peer-review system and the training of referees deserve attention and that potential problems with the number of manuscripts need to be addressed. More open to abuse by fraudulent investigators are reports of symposia sponsored by pharmaceutical or instrumentation companies, since such documents are usually not peer-reviewed. Who selected the points of view in a symposium report? Who paid for its publication? Was it the subscribers (who are presumably motivated by wanting to know the results of the studies) or the drug company that wants to sell a drug to practitioners?

Replication: Replication, once an important element in science, is no longer an effective deterrent to fraud because the modern biomedical research system is structured to prevent replication—no to ensure it. It appears to be impossible to obtain funding for studies that are largely duplicative. Indeed, proposed federal legislation (H.R. 1708) would prohibit federal funding for any work deemed duplicative by a new federal agency. In addition, academic credit tends to be given only for new findings. Furthermore, attempted replication will detect only incorrect results; it will not detect correct results based on fraudulent data. In this regard, the cases of fraud that have figured prominently in the recent literature fall naturally into two categories. Occasionally, as in the case of Slutsky, the culprit does not play for high stakes. The goal is simply to establish a niche in the profession, and to this end, the papers announce results that are predictable extensions of basic work done by others. This strategy is relatively safe, because the perpetrator can expect that attempts at replication (should they be made) would reach sufficiently similar conclusions. Provided that the fabrication of the data set does not leave any clues - for example, in the form of peculiar statistical properties - the dishonesty will go undetected. Because of the number of methods of measurement in cardiovascular research, this field lends itself to this type of fraud. Some of Slutsky's work involved measurement of the effects of drugs on myocardial ischemia using newly

developed techniques such as computerized tomography; the results were reliably predictable from work that had used older methods.

The example of Mark Spector illustrates the other type of fraud5, in which a scientist convinced of the correctness of a new idea wrongly gambles that others will be able to "replicate" an imaginary result. The dishonesty may be successful if the scientist's theory is indeed correct and others provide real data to support the fictitious or inflated findings originally claimed to support the new hypothesis. If others' attempts at replication fail, the scientist may be able to plead, as both Spector and Darsee did, that the experiments were too delicate to work unless the experimenter had extremely subtle skills.

We conclude that peer review and replication will not always detect fraud, and that trust in a researcher's motivation ignores the ever-present dangers of conflict between personal and intellectual motives. The question that arises is, How can extensive fraud escape discovery for so long in a university laboratory where the investigator is closely surrounded by scientists and investigators?

Who Are The Victims Of Fraud?

The quality of medical care and the direction of clinical research could be affected by the publication of misleading data, such as those concerning the efficacy, side effects, and safety of new pharmacologic agents. Research funds and efforts could be misdirected. Considerable time and funding were wasted as a result of fraudulent publications by Darsee and by Slutsky. Public revelation of research fraud damages the whole scientific community. This may take many forms - from subtly influencing the career choices of scientists in training to altering funding patterns of public and private agencies. Reports of fraudulent behavior could conceivably encourage further fraudulent behavior.

Coauthors at several levels have been affected by the Slutsky affair. Young investigators who were trained in the laboratory at the time of the fraud have had to remove collaborative publications from their bibliographies and to live with the onus of having worked with a perpetrator of fraud. Among the most vulnerable were younger faculty members who were independent investigators at the time the fraudulent work was

performed but whose careers were not firmly established. Senior faculty may have their future opportunities for positions or funding compromised.

Thus, research fraud affects the careers of coauthors with varying levels of responsibility or culpability. Although many coauthors failed to show proper responsibility in regard to statements made in their name, such responsibility would have required a bold initiative on the part of a new research trainee not familiar with the unwritten guidelines for the conduct and publication of research. More senior investigators had the means to exercise responsibility. One of the important roles of a full investigation is to establish the validity of publications in which the untrustworthy person had no fraudulent influence, so that coauthors of those papers will not be wrongly blamed on the basis of association.

How the University and the Scientific Community Fail Early Independence

Independence is prized in academic circles, but premature independence in research can limit the learning of good research practices. Slutsky asserted independence early in his research training by assumin complete responsibility for designing projects and writing reports. That would have been worthy of praise, provided that his research supervisors has continued to oversee his projects and publications. As much as possible, however, Slutsky avoided review of his work by others, and he even ignored requests by coauthors to see data and manuscripts. Changing his specialty-training areas (from cardiology to nuclear medicine to radiology) and laboratories several times seemed to help him to avoid close, knowledgeable supervision and formal academic review, although he may simply have been following the best opportunities for productive research.

We conclude that research supervisors should encourage initiative but not complete independence until an investigator has demonstrated good research practices and ethics, as well as technical expertise. While maintaining a collaborative relationship as independence grows, the senior scientist should ensure that young investigator's ideas are correctly attributed to them. Each university or department should develop guidelines for the supervision and evaluation of research trainees, as well as

a policy on publication and authorship. Perhaps clinical subspecialties should develop training guidelines for research and teaching careers that provide formal training in experimental design, statistical analysis, and basic science, and thus differ from training primarily for careers in patient care, or perhaps formal training in the evaluation of experimental data should be an integral part of clinical training.

Excessive Publication

High research productivity is encouraged and rewarded in most academic institutions, and Slutsky's role models in cardiology and radiology had a great many papers published. Slutsky's incredible productivity (one paper every 10 days while he was simultaneously a radiology resident and an associate clinical professor of radiology) failed to raise alarm. Indeed, unreasonably high productivity was applauded by some coworkers and supervisors. On the other hand, many did not recognize Slutsky's excessive productivity for two reasons. First, his papers were published in a variety of journals in three separate sub-specialty areas, so that no one group reviewed all his work. Second, Slutsky created an appearance of high collaborative productivity by putting the names of colleagues and students on papers to which they had made little or no contribution. He involved others in sharing responsibility for his research, whether valid or invalid, by means of this supposed favor. Some coauthors were not aware of a publication until they were contacted by the investigating committee. In some cases, Slutsky forged signatures on journal's copyright-permission forms. One new research trainee was presented with a publication in which she was listed as a coauthor upon her arrival at the laboratory. Some colleagues and trainees were either too flattered or too embarrassed to have their names removed from papers to which they had not contributed, and many lacked the authority or willpower to resist this practice. On the other hand, some faculty members expected their names to be used even though they had provided only the facilities for a project, without making a substantive contribution to the work or having a knowledge of its validity. Thus, gift co-authorship was effectively a disguise for an impossible amount of work by one person.

Finally, although Slutsky showed his early supervisors his data summaries, he frequently sequestered original data. He would not produce lists of names of patients in clinical studies. In regard to papers in which Slutsky was the first author, coauthors rarely recalled seeing written comments of manuscript reviews. In retrospect, coauthors regretted their permissive attitude toward these "eccentricities." Although this abrogation of the responsibility of co-authorship enhanced the speed of publication, it removed a mechanism to detect fraud and allowed Slutsky to alter reported results or procedures to satisfy reviewers' critiques. Many of Slutsky's coauthors never saw raw data or even data summaries. Although collaborative relationships between coauthors must be based on trust, some joint data review should be an integral part of the research process, even when unique laboratory procedures necessitate long- distance collaboration.

It is obvious that creativity and quality in research ought to be prized above quantity, but unfortunately, this is not often the case. Senior investigators must remain at least partly responsible for the quality of work of junior colleagues. Integrity regarding criteria for authorship must be as integral a part of scientific publication as is the accuracy of the data, and all coauthors should exercise their responsibility for the quality and accuracy of a scientific publication. At the time of publication each investigator could be asked to specify his or her role in the research, and records of their reports could be maintained by each department for review at the time of promotions. Journals could require similar statements to be submitted along with the copyright-transfer form. This practice should clearly identify gift authorship and thereby limit it.

Departments could require the statements of contribution and approval of all submitted papers by all coauthors - a practice that would allow young investigators to voice an opinion concerning their role in research at the departmental level. Senior investigators should not put their names on work unless they actually provided ideas and reviewed data. Co-authorship should reflect scientific involvement and responsibility for the work reported.

Excessive production should be scrutinized because of the deleterious effect on quality when quantity is great. New faculty and trainees should be clearly informed of realistic performance expectations by the laboratory, the department, and the university. Most important, the emphasis of quality must be both de jure and de facto in promotion, academic review, academic rewards, and peer review of funding.

Journals' Responsibility

The review of publications by some journals was ineffective in detecting discrepancies in procedures and statistics, as well as implausible technical accomplishments. It might help if tables of data were submitted to reviewers, even if those tables were not to be included in the published article, so that the reviewer could judge whether a figure was a fair representation of the data. In view of the evident limitations of the peer-review system in detecting internally consistent fabrications and the necessity for trusting the integrity of scientific reporting, journal editors seem relatively powerless to prevent the type of fraud encountered in the Slutsky case.2 At a minimum, however, all journals should require signed approval of final manuscripts by all coauthors and should perhaps communicate with separately with each coauthor.

Journals play an important part after has been established. In both the Darsee and Slutsky cases, large numbers of publications and several coauthors were involved. Coauthors and institutions clearly share the responsibility for the reevaluation of the published work of an investigator shown to have committed fraud, and journals must be responsible for publishing the results of appropriate investigations. Not to review the work and not to publish the results of this is, in our opinion, an abrogation of responsibility. To a large extent both the Slutsky and the Darsee investigations have failed in this respect.

Very little of the investigation of Darsee's bibliography was made public.4 Although a committee investigated all of Slutsky's publications, this review was not an evaluation of scientific content but a search for direct evidence of fraudulent data and opportunities for Slutsky tc alter data in a paper. Since some journals are reluctant to publish retractions for legal or other reasons, publication of the committee's findings has been incomplete. For example, Circulation and other American Heart

Association journals will publish retractions only if they are agreed to by the authors - a policy that does not deal effectively with fraudulent behavior and ignores the possible conflict of interest of coauthors.

Failure to Report Suspicions

Although some of Slutsky's junior and extra-departmental colleagues (and even one journal editor) harbored suspicions about the validity of submitted manuscripts, they failed to communicate their concerns to the responsible department official (the chairperson), the responsible School of Medicine official (the associate dean), or other supervisors. Hints about unreasonable research accomplishments or careless techniques in letters of recommendation for Slutsky's appointment were never pursued by the Department of Radiology. When department members considered Slutsky's faculty appointment, they remained uninformed about concerns in the cardiology division about Slutsky's carelessness and unwillingness to share data. Furthermore, such concerns did not inhibit some of Slutsky's colleagues from having nominal collaboration with him.

Faculty and trainees should be informed of the policy and procedures for reporting on unethical research practices. It should be considered an academic obligation for faculty to inform supervisors if they have reservations about the integrity of the work of a faculty member or fellow.

Failure to Retain Data

Slutsky's original data simply could not be found, even in cases in which the studies had clearly been performed. There were not university regulations governing retention of research records and data. All university should establish standards and facilities for data retention.

Conclusions

Our observations in this case lead us to conclude that research fraud, although probably rare in proportion to the large numbers of scientific publications, may escape detection or investigation and that there are scientists prepared to run the appreciable risks associated with making inaccurate statements in papers that bear their names. sophisticated dishonesty may not be detected by peer review or attempts to confirm results. Personal motives can lead researchers to make careless errors or to practice minor dishonesty or even outright fraud. Moreover, the emphasis on competition and the constant pressure to produce , while intended to advance the discovery of truth, are likely, in the present context, to foster conflict between personal career goals and the intellectual motivations of science.

Solutions to prevent research fraud are clearly needed but cannot be adopted quickly; to institute practices that might stifle originality or discourage co-operative research would be counter productive. Solutions to the problems of scientific fraud and irresponsibility require a more refined taxonomy of case, greater understanding of the harm they cause ("Does it really matter," the cynic asks, "if a cunning faker gets away with producing hundreds of papers that nobody reads?"), appreciation of the costs of changes in the structure of scientific research, and careful consideration of the goals that the scientific community seeks to achieve.

Appendix Dr. Slutsky's Bibliography

1. World J Surg 1978; 2:825-6. Valid
2. Exerc Sport Sci Review; 6:105-23. Valid
3. Radiology 1979; 132:155-9. Valid
4. Am J Cardiol 1979; 44:459-65. Questionable
5. Circulation 1979; 556-64. Valid
6. Circulation 1979; 565-71. Valid
7. Am J Cardiol 1979; 44:1365-70. Valid
8. J Nucl Med 1979; 20:491-5. Valid
9. Am J Cardiol 1979; 44:318-24. Valid
10. Circulation 1979; 60:1004-13. Valid
11. Cathet Cardiovasc Diag 1979; 5:305-17. Valid
12. Radiology 1980; 135:725-30. Questionable
13. Circulation 1980; 61:1083-90. Questionable
14. Am J Cardiol 1980; 45:63-71. Questionable
15. Am J Cardiol 1980; 45:831-40. Questionable
16. Am J Cardiol 1980; 46:813-30. Questionable
17. Am J Med 1980; 68:197-205. Valid
18. Am J Cardiol 1980; 45:197-202. Valid
19. Clin Cardiol 1980; 3:14-18. Valid
20. Cathet Cardiovasc Diagn 1980; 6:49-60. Valid
21. Circulation 1980; 61:1004-9. Valid
22. Arch Phys Med Rehabil 1980; 61:517-22. Valid
23. Eur J Cardiol 1980; 11:425-34. Valid
24. Aviat Space Environ Med 1980; 51:892-8. Valid
25. Radiology 1980; 136:197-201. Valid
26. Chest 1981; 80:471-80. Questionable
27. Am J Cardiol 1981; 47:357-64. Valid
28. Circulation 1981; 63:864-70. Questionable
29. Am Heart J 1981; 101:556-60. Questionable
30. Chest 1981; 79:501-5. Questionable
31. J Nucl Med 1981; 22:506-9. Valid
32. Am Heart J 1981; 101:414-20. Valid
33. Radiology 1982; 143:623-6. Valid
34. An Heart J 1982; 104:100-4. Valid
35. Invest Radiol 1982; 17:241-8. Fradulent
36. Clin Cardiol 1982; 5:647-52. Fradulent
37. Am Heart J 1982; 104:27-35. Valid
38. Am Heart J 1982; 104:254-62. Valid
39. Radiology 1982; 144:623-5. Valid

40. Radiology 1982; 144:461-9. Valid
41. Br Heart J 1982; 48:39-47. Valid
42. Am Heart J 1982; 104:1027-32. Valid
43. Cathet Cardiovasc Diagn 1982; 8:597-606. Valid
44. Eur J Nucl Med 1982; 7:553-8. Valid
45. Invest Radiol 1982; 17:233-40. Questionable
46. Invest Radiol 1982; 17:362-6. Questionable
47. Am J Cardiol 1982; 50:874-80. Questionable
48. Eur J Nucl Med 1982; 7:6-10. Valid
49. Radiology 1982; 142:681-6. Valid
50. Am J Cardiol 1983; 51:43-51. Valid
51. AJR 1983; 140:425-30. Valid
52. Comput Biomed Res 1983; 16:88-98. Valid
53. Am J Cardiol 1983; 51:1218-22. Valid
54. Invest Radiol 1983; 18:130-7. Valid
55. Am Heart J 1983; 105:940-5. Valid
56. Radiology 1983; 147:659-62. Valid
57. Am J Cardiol 1983; 51:1399-403. Valid
58. Circulation 1983; 67:1245-51. Valid
59. Crit Care Med 1983; 11:913-7. Fradulent
60. Am J Cardiol 1983; 51:1739-43. Valid
61. Invest Radiol 1983; 18:406-14. Valid
62. Invest Radiol 1983; 18:415-8. Valid
63. Heart Lung 1983; 12:522-3. Valid
64. Circulation 1983; 68:628-35. Valid
65. Radiology 1983; 149:371-4. Valid
66. Cardiovasc Intervent Radiol 1983; 6:252-62. Valid
67. Radiology 1983; 146:219-21. Questionable
68. Am Heart J 1983; 105:53-9. Questionable*
69. Am Heart J 1983; 105:243-8. Questionable
70. Circulation 1983; 67:759-65. Fradulent
71. Circulation 1983; 68:164-9. Questionable
72. Invest Radiol 1983; 18:33-9. Questionable
73. Am J Cardiol 1983; 51:468-75. Questionable
74. Am Heart J 1983; 105:802-10. Questionable
75. Cardiovasc Res 1983; 17:113-21. Questionable
76. Invest Radiol 1983; 18:122-9. Questionable*
77. Cardiovasc Intervent Radiol 1983; 6:7-13. Questionable
78. Am Heart J 1983; 106:107-13. Questionable
79. Am Heart J 1983; 106:41-5. Questionable

80. Comput Biomed Res 1983; 16:234-46. Questionable
81. Invest Radiol 1983; 18:327-34. Questionable
82. Chest 1983; 84:2-3. Questionable
83. Cardiovasc Intervent Radiol 1983; 6:154-9. Questionable
84. Crit Care Med 1983; 11:721-5. Questionable
85. J Surg Res 1983; 35:181-7. Questionable
86. Invest Radiol 1983; 18:27-32. Questionable
87. Radiology 1983; 149:375-78. Questionable
88. Eur J Nucl Med 1983; 8:523-7. Questionable
89. J Nucl Med 1984; 25:38-41. Valid
90. Am J Cardiol 1984; 53:228-33. Valid
91. Am J Cardiol 1984; 53:282-9. Valid
92. Clin Cardiol 1984; 7:37-43. Valid
93. Radiology 1984; 151:145-8. Valid
94. Am J Cardiol 1984; 53:929-35. Valid
95. Am Heart J 1984; 107:733-7. Valid
96. Am Heart J 1984; 108:1292-7. Fradulent
97. Invest Radiol 1984; 19:129-32. Valid
98. J Comput Tomogr 1984; 8:133-6. Valid
99. Am J Cardiol 1984; 53:1547-52. Valid
100. Invest Radiol 1984; 19:202-7. Valid
101. Cardio Intervent Rad. 1984;19:Supp.3:S1-S7.Val
102. Am Heart J 1984; 108:159-64. Valid
103. Am Heart J 1984; 108:548-53. Valid
104. Am Heart J 1984; 108:591-7. Valid
105. J Comput Assist Tomogr 1984; 8:739-44. Valid
106. Invest Radiol 1984; 19:374-9. Valid
107. J Urol 1984; 132:1216-9. Valid
108. J Cardiovasc Pharmacol 1984; S757-S761. Valid
109. J Am Coll Cardiol 1984; 3:138-42. Fradulent
110. Am Heart J 1984; 108:1431-6. Valid
111. J Am Coll Cardiol 1984; 3:1454-60. Fradulent
112. Clin Cardiol 1984; 7:99-101. Questionable
113. J Nucl Med 1984; 25:581-91. Questionable
114. Cardio Intervent Rad. 1984; 7:59-64 Questionable*
115. Invest Radiol 1984; 19:168-73 Fradulent
116. Am Heart J 1984; 108:90-6 Questionable
117. Circ Shock 1984; 13:183-91 Questionable*
118. Am Heart J 1984; 108:401-7 Questionable*
119. Am Heart J 1984; 108:543-7 Questionable*

120. Radiology 1984; 152:575-8 Questionable*
121. Radiology 1984; 153:317-20 Questionable*
122. Invest Radiol 1984; 19:510-6 Questionable*
123. Magn Reson Imaging 1985; 3:37-42 Valid
124. J Am Coll Cardiol 1985; 5:273-9 Fradulent*
125. Chest 1985; 87:392-3 Valid
126. Radiology 1985; 155:11-4 Fradulent*
127. Radiology 1985; 154:727-31 Valid
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